The Cryosphere Discuss., 7, C3472–C3485, 2014
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The Cryosphere
Discussions
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Interactive comment on "A 10 yr record of black carbon and dust from Mera Peak ice core (Nepal): variability and potential impact on Himalayan glacier melting" *by* P. Ginot et al.

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Received and published: 7 April 2014

⇒ In this document, I have copied Referee #1 comments, and answered them in the lines with →

The paper, "A 10 yr record of black carbon (BC) and dust from Mera Peak ice core (Nepal): variability and potential impact on Himalayan glacier melting", by Ginot et al. firstly reported the ice core proxies obtained from the southern flank of Nepalese Himalaya, which include the important light absorbing impurities (dust and BC) on snow albedo discussion. The paper was overall well written and the ice core data were worth publishing. However, I have some major concerns and the authors need to clarify these

points before the publication.

(Major concern)

1. They measured BC mass concentration in the ice core using SP2 with the nebulizer. As in detail discussed on the accuracy of the measurement by Jenkins et al. (TCD, 2013; see at: http://www.the-cryosphere-discuss.net/7/4855/2013/tcd-7-4855-2013.html) and Kaspari et al. (ACPD, 2013; see at: http://www.atmos-chemphys-discuss.net/13/33491/2013/acpd-13-33491-2013.html), the sample treatment before the analysis and the measurements of BC with SP2 using the nebulizer had larger uncertainty (very low accuracy). That is why they focused on "relative differences" rather than absolute concentrations though they still discussed the absolute numbers in the current form and they are problem and need revisions. In this study, the authors, however, only used a simplified correction factor of 0.56 and did not consider and discuss such lower accuracy of the SP2 measurement for liquid-phase samples (i.e., melt water samples) with sample treatment accuracy before the analysis. Based on Jenkins et al. (2013) and Kaspari et al. (2013), I do not believe that using such a simplified correction works well for the absolute numbers of rBC concentration in the ice core samples. The authors need to show the validity of using the simplified correction factor to fully correct the rBC mass concentration in the ice core data. If they cannot show, they also need to show the relative numbers in their table and figures of this study. Then, they also should not use the absolute number of BC for the snow albedo and radiative forcing calculations (i.e., only keep discussing dust snow darkening but the discussion of BC snow darkening is difficult in this study in terms of absolute number). Even if they cannot discuss such calculations, still it is worth showing the relative numbers in figures for references. However, SO AS NOT GENERATE MISUSING OR MISLEADING BY ANY READERS, the authors strictly need to choose the decision whether they can use the absolute number of rBC for the discussion or not. If the absolute number will be shown in the published table and figures under the meaning less absolute numbers, readers in cases just grab the numbers to use their papers and that will further generate misusing

and misleading. Therefore, this point is the most important comment of my comments. The discussions with Jenkins et al. (2013) and Kaspari et al. (2013) are essential in the revision, in which Kaspari et al. (2013) obtained snow samples from the same Mera Glacier. The discussion of this study with Kaspari et al. (2013) would provide more information of snow darkening over the Mera Glacier.

⇒ My manuscript was accepted on TCD on 13 December 2013, before Kaspari et al. who was released on ACPD on 23 December 2013. With such a timing, it was not possible for me to include them in the discussion. However, now and in order to prepare a revised version of my manuscript, I will take this paper in consideration as well as Jenkins et al (2013) study.

⇒ Concerning the accuracy of rBC content in snow samples, we have to split the answer in two steps. A) the measurements accuracy using a coupled Nebulizer/SP2, and B) the sample BC content changes due to transport and storing processes. All these points and our experiments to provide the best accuracy for our measurements are well described in Lim et al. (2014) published recently in Atmospheric Measurement Techniques Discussion.

⇒ Measurement accuracy using a coupled Nebulizer/SP2: from (Lim et al, 2014). This paper describes precisely the method for the SP2 calibration (§2.1.2), but also the aerosolization of rBC particles in liquid samples (§2.1.3 from Lim et al.). This protocol allows to give the precise "measured BC concentration (MrBC)" (in comparison with Kaspari et al. MBC values)

Lim, S., Faïn, X., Zanatta, M., Cozic, J., Jaffrezo, J. L., Ginot, P., and Laj, P.: Refractory black carbon mass concentrations in snow and ice: Method evaluation and inter-comparison with elemental carbon measurement, Atmos. Meas. Tech. Discuss., 7, 3549-3589, 10.5194/amtd-7-3549-2014, 2014. http://www.atmos-meas-tech-discuss.net/7/3549/2014/amtd-7-3549-2014.html

⇒ For the Mera Glacier sample analyzes, the nebulizer efficiency (56%, measured

each day) was measured using the same protocole. This efficiency coefficient correction was applyed to the measurements to obtain the MrBC values.

⇒ The second point is to correct the measurements from the sample BC content changes due to transport and storing processes. This point was also well studied and the results are described in Lim et al. (2014). Several tests were performed for different type of sample containers, for the effect of storage and procedures after melting and we concluded to an appropriate protocol and correction factor to calculate the "absolute BC concentration (rBC)" from the "measured BC concentration (MrBC)".

⇒ We have since the reviewer comments performed additional test for the Sampling/Melting/Freezing/Melting/Freezing/Analyze procedure using Col du Dôme summer surface snow. For this case, the lost betwwen MrBC and rBC is 59±8% (3 samples).

⇒ In the submitted manuscript, the nebulizer efficienty (56%) was used to correct the SP2 measurements. This values correspond to MrBC from (like in Kaspari manuscript)

⇒ However we did not apply in the submitted manuscript an additional correction for the sampling/storing processes. In a revised manuscript version, we will also apply this second correction factor (59±8% lost) in order to reach absolute rBC (rBC) snow concentration. Both values can be included in the revised version, in the results tables but also for the modeling as a maximum rBC amount (correction 59+8%).

⇒ However, if both corrections are applied, the average rBC absolute concentration (rBC) from the ice core is 9.1 μg l-1 (145 μg l-1 maximum).

⇒ At Mera col, the same site than our ice core, Kaspari et al. (2013) reported average and maximum MBC values of 1.0 and 8.4 μg l-1 respectively, in agreement with our MrBC values (Table 1: 3.0 and 47.90 μg l-1 as mean and maximum value respectively). Our maximum value is still larger but our ice core covers 10 years, and rBC has a large temporal variability, explaining that maximum values higher than the value reported on a single day by Kaspari et al (2013) are not inconsistent.

2. In Section 4.3 and 4.4, they estimated glacier melting based on the proxy information with calculation and energy balance model. However, I do not think the application of ice core proxies obtained from an accumulation zone to the ablation zone is meaningful because the proxy condition would be quite different. This further gets readers confused and in cases generates misleading. If the authors would like to discuss the glacier melt amount with dust and BC concentrations, the observations like Kaspari et al. (2013) in the ablation zone should be necessary. If the authors discuss the impact of the proxy in the accumulation zone with the same method, this would be fine.

⇒ Absolutely right, we can not apply the impurities concentration mesured at 6400m to the ablation area where all impurities are collected on the glacier surface since long time or washed out by melt water flowing. This question was addressed in the original manuscript (P6021L26-28)

Together with above, the authors need to re-consider the discussion in Section 4.4.

⇒ The whole section was revised

In addition, the method of energy balance calculations was not mentioned in details and I could not follow how they calculated. Please explain in details how to calculate. Also, the authors need to summarize the mass balance using a schematic showing from the ablation to accumulation zone of the glacier. Only mentioning the numbers with sentences is difficult to see the whole structure of the mass balance. In addition, I could not understand "effective melting" at P6023L1. Please explain this in details.

⇒ We add: "Using meteorological data collected by the automatic weather station (AWS) at 5360 m a.s.l., the surface energy balance has been calculated at point-scale to derive the local melting and sublimation over the one-year period November 2009 to October 2010, following Wagnon et al. (2003) methodology. The four components of the radiative balance have been measured directly in the field using a CNR1

Kipp&Zonen sensor. The sensible and latent heat fluxes have been computed with the bulk aerodynamic approach including stability correction, using one single level of measurements of air temperature, relative humidity and wind speed. The other terms of the surface energy balance (heat supplied by precipitation, and conductive heat flux inside the glacier) are negligible during the melting season compared to the other fluxes. At hourly time step, when surface temperature reaches 0°C, the sum of the radiative and turbulent heat fluxes, when positive, gives the amount of energy available for melting. When directed away from the surface, the turbulent latent heat flux is a sublimation flux. Consequently, melting and sublimation could be computed at hourly time step. Between November 2009 and October 2010, the total annual ablation at 5360 m a.s.l. was 3690 kg m-2, including 3550 kg m-2 of melting and 140 kg m-2 of sublimation (only 4% of the annual ablation, which is thus insignificant compared to melting)."

⇒ We add: "Annual accumulation is not known at 5360 m asl but is likely to be higher than net accumulation measured at the highest part of the glacier (6330 m asl) which experiences only a limited ablation only through sublimation and wind erosion, and in turn collects almost all the annual snow accumulated at the glacier surface. In 2009-10, this net accumulation was , whereas energy balance approach results in an annual melting of 4300 kg m-2 y-1 melting without taking into account sub-layers refreezing. Considering that in 2009-10, the net accumulation measured at the highest part (6330 m asl) of Mera Glacier is 720 kg m-2 (Wagnon et al., 2013). As a consequence, , annual accumulation at 5400 m asl is at least 720 kg m-2 making the effective melting ablation at 5400 5360 m asl is higher than 3000 kg m-2 (=2280+720). Given that sublimation is insignificant compared to melting for annual ablation, tThe correct true melting value is in turn comprised between these previous 3000 and 4300 3550 kg m-2 values obtained with mass balance measurements and energy balance approach respectively. The melting related to impurities calculated in this study for the same year 2009-2010 corresponds to +543 kg m-2 y-1 annual melting associated to dust and rBC, and +215 kg m-2 y-1 for rBC only."

⇒ “effective melting” stands for the “true melting”. Indeed, comparing the melting computed with the SEB approach, and the ablation derived from point-scale mass balance data (stake readings), we can conclude that the true melting at point-scale is comprised between 3000 and 3550 kg m-2.

(Minor points)

P6004L14; P6007L2-6: References lacked.

⇒ e.g. Bonasoni et al., 2010

⇒ Bonasoni et al., 2010 ; Barros and Lang, 2003

P6004L29: They mainly discussed dry depositions.

⇒ If Yasunari et al. mainly discuss dry deposition, order studies estimates BC wet deposition (e.g; Ménégoz et al. 2014, Hienola et al., 2013; Nair et al., 2013)

P6005L14-27: The authors should include the discussions from the latest papers by Jenkins et al. (2013; see above), Kaspari et al. (2013), see above), and Ménégoz et al. (2013; ACPD; see at: http://www.atmos-chem-phys-discuss.net/13/31013/2013/acpd-13-31013-2013.html).

⇒ For Jenkins et al., a final paper in TC is not foreseen.

⇒ Kaspari et al. present some interesting results from the same glacier. If some critical remarks were done by the reviewers on the data quality, I'm convinced that the conclusion on the altitudinal gradient for the BC deposition fluxes is real but not quantified with this work.

⇒ I've included Ménégoz et al., 2013

P6007L25-28: How did the authors decontaminate the surface contaminations of the ice core samples? Removing the surface? Using ultrapure water?

⇒ The firn core was extracted with a small electromechanical drill (small FELICS, see

Ginot et al, 2002). This drill is composed, for the core container, by a simple aluminum barel. This tube was pre-cleaned with ultrapure MilliQ water before the field campaign. As tested before, we can cut the core samples into the pre-cleaned vials directly when extracting the core from the drill barrel, without additional decontamination. This procedure is validated for major ions, dust and BC. Of course, not for other sensitive organic acids. . .where external decontamination is necessary and performed in the lab coldroom.

P6008L15: Do you have a reference to determine the dust density?

⇒ We used the density of mean crustal material (2.5 g cm-3) usually used for ice core analyzes (e.g. Delmonte et al., 2002).

P6012L25-28: This is an important result and should be in the abstract too.

⇒ OK

P6013L13-25: The figures on the EOF analysis should be shown such as the scores in time series of each proxy. They can provide this as supplementary information.

⇒ Whether I keep the EOF analyzes in the manuscript with a developed description on the aerosol sources description and in that case I can add the requested figures and other information in supplementary information, or I can remove this EOF analyzes.

P6014L4-5: This is also an important result and should be in both the abstract and conclusion.

⇒ OK

P6015L11: The smaller numbers should be spelled out (i.e., two rather than 2).

⇒ Done

P6018L25-26: Although the dust particle size distributions were shown in Fig. 3, no size distributions for rBC were shown. The authors should add the size distributions for rBC too.

⇒ I combined figure 3 with dust and BC size distributed is in preparation using the following data measured by SP2 for two seasons (mean of several samples)

⇒ See Fig 3b included

P6020L4-15: Very hard to follow the method of simulation. More detailed explanations are necessary. For example, did they only calculate one-day simulation for each month? Did the one-day simulation include hourly calculations? They can use supplementary information to explain these in details.

⇒ The new simulations were indeed performed for the 1st and 15th of each month. For each of these days, the daily absorbed energy was computed using simulations at 5 :00, 8 :00, 10 :00, 12 :00, 14 :00 and 16 :00 LT. The daily absorbed energy was then reconstructed using interpolation between these two days of each month. A more detail description of the method for simulations have been included in the revised version of the manuscript.

Table 1: The authors need to emphasize that the data were obtained from the accumulation zone of the glacier here too.

⇒ Corrected with "Table 1 : Data summary:(EOF analyzes parameters and aerosol concentration and fluxes obtained from Mera glacier ice core."

Fig. 2: If the authors cannot discuss the absolute number of rBC, replace the rBC data to the relative numbers. Dry season should be pre-monsoon season because the dotted lines correspond to the peaks of oxygen isotope.

⇒ I plot both MrBC and rBC. The same for all tables results

Fig. 3: How did the authors determine the peak size?

⇒ Visualy. The difference between each curve is large and the estimated value with 0.1μm resolution is enough.

Fig. 4: The gray line for PM is hard to see. Please change the color.

⇒ New concept for figure 4

Fig. 5: Very hard to distinguish each by each. Please separate the cases of coarse and fine snow grain size. Then, four figures should be better.

⇒ This has been changed in new figure 5

Interactive comment on The Cryosphere Discuss., 7, 6001, 2013.

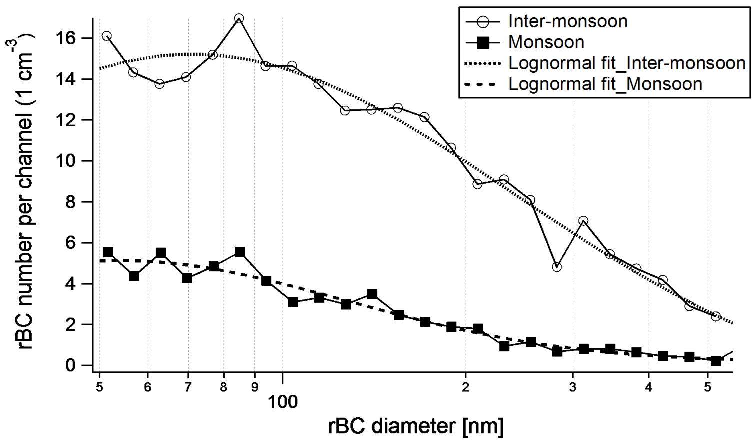
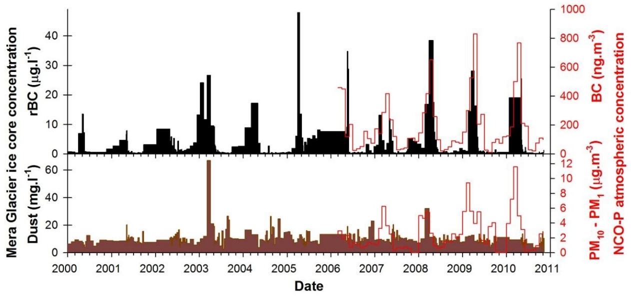


Fig. 1. New Fig 3b



(Data for the new figure 4 to finalize)

Fig. 2. New Fig 4

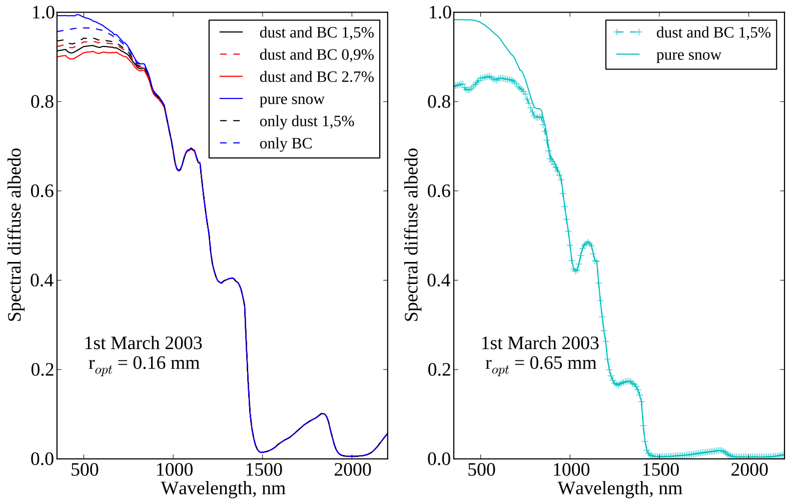


Fig. 3. New Fig 5 ab

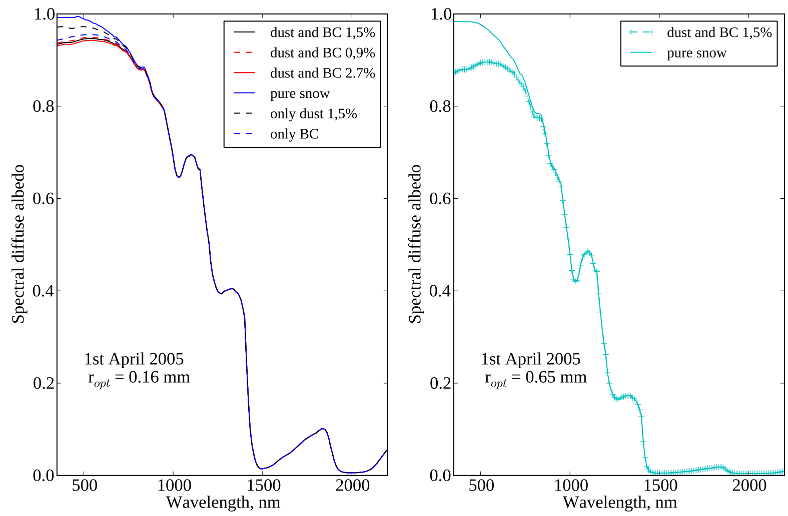


Fig. 4. New Fig 5 cd